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ABSTRACT

Three obstacles appear to frustrate the potential contribution of empirical research and social science to policy formulation. First there is the inherent conflict in the political decision making process; second, reality is so complex it defeats our ability to locate the sphere of understanding which we should apply; third, knowledge presupposes a framework to interpret it; but in a pluralistic system there are only competing frameworks. The work of Kuhn may be relevant for understanding the contribution of social science research to established social policy paradigms. In this paper, the resistance of personal paradigms to change is illustrated, as well as the stubborness of categories of thought despite alternatives suggested by research. Skeptics take this as evidence that only a weak link is possible between research and policy. Optimists argue that such a conclusion is premature. Research undertaken within a common framework of thought can both by contention and concensus influence the development of policy; moreover, research and theory can help reshape policy paradigms as well, but their contributions cannot be isolated from the many other forces that impinge on decisions. The final section illustrates how prior-held frameworks shape the ideals sought (the skeptic's view), the theories of action proposed, and the evaluations undertaken. (Author/JM)

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# VALUES, SOCIAL SCIENCE, AND SOCIAL POLICY

Martin Rein

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Joint Center for Urban Studies of M.I.T. and Harvard

# VALUES, SOCIAL SCIENCE, AND SOCIAL POLICY*

Martin Rein

## The Relationship Between Reason and Purpose

It is almost univerally acknowledged that rational thought should inform the development of public policies. The term "rational thought" may not acutally be used, but the view that policy should be the consequence of rational review is so thoroughly accepted that it is not seen to deserve notice. Yet, implicitly or explicitly, the assumption remains.

Often this assumption is associated with the set of beliefs we might identify as the "science and technology" dogma. According to these beliefs, we have a field of study of reality, science, which produces fundamental understandings, and then another field concerned with practical applications, technology, which solves problems of immediate human concern through the application of the appropriate elements of our fundamental understanding. In the area of social policy, according to this view, we must develop an understanding of social reality so that we can subsequently apply that understanding to the development of social policies. Research should be directed at increasing our fundamental understanding of social conditions and the processes that create them. Although the proper interpretation and application of that under-

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standing may call for unusual experience, insight, and powers of invention, the research findings will contain the materials on which the application can be based. Administrative and political problems may inhibit the process but, in principle, it should be possible to complete the sequence of research leading to understanding and of understanding leading to recommendation. Or, in a more sophisticated form of the science and technology view, we may think of the process as iterative and believe that we can move from research to initial understanding to application, to more research, to further understanding and new application, to recognition of new problems, to still more research, etc. In short, a procedure can be established which permits learning from new experiences.

These assumptions are not so much false as overstated. The assumptions are plausible, and so they contribute to inevitable frustration when an increase in knowledge is not, after all, accompanied by an increase in our ability to design and change policy.

This frustration leads to a new skepticism about our ability to propose rational prescriptions on the basis of a dispassionate analysis of the nature of the problems and an evaluation of the limitations of present programs to cope with them. Alice Rivlin, after attempting to evaluate the usefulness of the PPBS system (Planning-Programming-Budgeting System) introduced in the federal government, concluded that "So far analysts have probably done more to reveal how difficult the problems and choices are than to make the decisions easier."[1] The efforts to make use of

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[1] Alice M. Rivlin, Systematic Thinking for Social Action, Washington, D.C.: The Brookings Institution, 1971.

systematically gathered and scientifically based information in policy decision making have revealed only how difficult it is to make choices when policy requires the synthesis of multiple, equally valued but conflicting objectives. A recent statement by Ida Merriam, Assistant Commissioner of Research for the Social Security Administration, expresses the frustration even more pointedly: She writes: "There is a growing recognition that much of the federally supported extramural research, particularly in the social sciences, has added little or nothing either to basic knowledge or to practical decision-making."[2] This view is widely held. Whether it is in fact valid must itself be the subject of further study.

Three obstacles appear to frustrate the potential contribution of empirical research and social science theory and knowledge, more broadly defined, to policy formulation. First, there is the inherent conflict in the political decision-making process: different people want different things and make use of the political process to satisfy their respective interests; second, reality is so complex it defeats our ability to locate the sphere of understanding which we should apply; third, knowledge presupposes a framework to interpret it; but in a pluralistic system there are only competing frameworks, and social science findings are seldom so conclusive that they permit a firm choice among competing perspectives. I want briefly to call attention to the first two types of obstacles and then to elaborate on the third.

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2 Ida Merriam, "Social Security Research: The Relation of Research and Policy Planning in a Government Agency," Journal of Social Policy, Vol. 1, Part 4, October 1972, p. 290.

Democracy and Rational Decision-Making

The conflict between rationality and democracy derives from the widely held view, at least among American political scientists, that there is no public interest which can be distinguished from sectional and group interests. "The grounds advanced by the...group theorists for denying the existence of the public interest are that as any proposal which becomes practical politics is opposed by some group, there cannot therefore be a single public interest."[3] In democratic societies there are only conflicting interests, each seeking to maximize its influence through the political process. Even the desire by social scientists to make use of rational analysis in influencing public policy must also be interpreted as a form of partisanship, one rooted in the desire to win more influence for academics and, correspondingly, to weaken the influence of other groups who now shape policy.

To make this general discussion about interest groups and information more concrete, we may consider the following situation.

Policy concerned with the building of atomic generating plants, so long as this remains an economic issue, can be decided by reference to projections of future energy needs and by consideration of economies and diseconomies. There will, of course, be interest groups whose own economic stake is dependent on the utilization of one sort of fuel or another, or on continued expansion of energy resources, or the like. But the trade-offs will be fairly clear to begin with, or will become so after a while, and compromise may reached. If not, then the conflict will, at least, be between clearly

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3 L. J. Sharpe, "American Democracy Reconsidered," 1972 (mimeo), p. 48.

identified adversaries over clearly defined issues. There is a procedure for resolving conflicts of efficiency which is generally accepted. But when construction of the atomic generating plant takes on social implications, when it is recognized that the plant will contribute new danger to its environs or raise issues about the social consequences of various waste disposal programs, the matter becomes more complex. Now there are many more parties than can easily be dealt with; they include all those who were previously engaged and, in addition, defenders of the neighborhood or community, environmentalists, and the like. Compromises become extraordinarily difficult to work out, partly because so much of the cost, to some of the parties at least, is in their anticipated quality of life. It helps, of course, to have as much understanding as possible of the reality under debate; but the debate cannot be ended by this additional understanding; and since the debate becomes more complex, and the competing values and interest groups become more visible, the issue may appear insoluble. Nor could it have been entirely anticipated that the pollution potential of the enterprise would become a salient public issue. Economic gain and loss is a persistent concern, but beliefs about what determines the quality of our lives are constantly subject to change and redefinition. There is no consensus about how to sum up the costs and benefits to different groups or the weights to assign to economic gains and social externalities.

There is an intractable conflict of purposes between political decision making, which is about the power of competing interest groups. and analytic research, which is about rational problem solving. Rationality does not, in this context, imply that some definable logical procedure has been followed which has exhaustively scrutinized

all possible options or considered all relevant information. Rather, it suggests that, at the least, the process of making a decision intelligibly made use of whatever resources of knowledge, judgment, imagination, and analysis were available in the circumstances. The worrying question, is, nevertheless, whether the political system is capable of acting intelligently in some nonpartisan sense? Or is the whole notion of impartial intelligence inapplicable because there is no common good or established means of determining it?

One suggestion for the resolution of this problem is to distinguish between questions of implementation and questions of policy. Questions of implementation assume that the objective is given and ask only how to reach it, or what other unintended consequences might follow from the pursuit or achievement of the accepted objective. Questions of policy, by contrast, ask for an ordering of goals or objectives. These questions are less amenable to dispassionate understanding.

But efforts to distinguish between ends and means are often superficial. The consideration of one requires elimination of the other. There is, then, not much to be gained from efforts to delineate the terrain of policy and research or to treat them as separate worlds. Although conceptually they may be distinct from one another, there is a natural interplay between them when they operate in the real world.

Other efforts to integrate research and the political process start at the other end and argue that research must accommodate itself to the political process. The task is first to understand better how decisions are taken in government, and then, based on

this understanding, determine where and how in the process research can best be introduced. Research must therefore be more flexible and resilient if it is to be of use to political leaders and administrative heads.

Still others take the view that "There can be no agreement either on goals or on societal relationships that link program inputs to sought-after outputs.... American society is inherently incapable of being goal-oriented for deep-seated ideological reasons; accordingly, applied [illegible]tic rationality cannot be the basis of social accounting."[4] Aside from id[illegible]ssues, the extraordinary difficulty of reconciling and weighting multiple [illegible]ming interests limits the potential for rational policy making. We must accept, therefore, as the best solution whatever the political process produces. But sometimes those who hold this view go farther by identifying the political process itself with rational decision making. The disjointed-incremental school holds that when certain conditions are met, such as representation of all interested parties in the political process, the outcome must yield a rational decision. This is what Mannheim termed "procedural rationality." But this view is also vulnerable. Though the process may be just, its product may still be both unjust and unrelated to the knowledge base.

## The Complexity of Social Phenomena

Even where there are no immediate clashes of interest, implementation of research findings may still be problematic because of the complexity of the situation for which

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[4] Brian J. L. Berry, "Social Accounting Systems: Problems in Conceptualization and Realization," a paper presented at the annual meeting of the National Academy of Sciences, May 1972.

policy is being proposed. Head Start offers an example. It is not at all clear where the roots of academic competence are to be found, or how certain is the link between academic achievement and future earnings; and so the underlying theory of the program is uncertain. But even assuming we knew that academic competence begins with very early exposure to academic tools, and we accepted such competence as a good in its own right, we would still have to know how schools, teachers, families and peers impinged on Head Start students in order to know how Head Start itself, as an application, worked. This is not to claim that the program does not work or should not have been tried. It does suggest, however, that in social areas the success of an application depends on the impact of diverse factors in addition to the one we regard as fundamental.

In this context, it may be useful to distinguish between the kinds of laws which emerge from physical laboratories and, in turn, which guide applied research in the industrial laboratories, as opposed to the kinds of understandings which come out of the sociological study of real situations, but which rarely lead to the formulation of regularities.[5] Instead, the latter tend to describe the functioning of what Lawrence Frank has called "organized complexities." These complexities are affected by external events, and so long as the externalities are unknown, there will continue to be uncertainties about the future even though there may be full knowledge about the past.

In no area of study can inferences based on our past research be applied to a situation that has "significantly" changed. And the likelihood of significant change in

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[5] Such regularities do exist; for example, some have been identified in relation to the dependence of young children on attachment figures for healthy development. This example has important applications that are not immediate -- for instance for personality in later life, and for the problems early deprivation gives rise to.

social areas seems unusually great. The only means to determine whether the new situation is comparable to the old one has to be applied ex post facto. For this reason, knowledge in the policy arena is not entirely self-correcting. Scientific research requires that one continue to experiment until the experimental results and the theory correspond, with the data serving as a correction to the theory and the theory as a guide to the experiment. But in policy oriented research, there is no way of anticipating dramatic changes in the social context which will have serious consequences for the implementation of whatever policies are selected. A reasoned argument based on past situations must surrender to the uncertainty of future events, thus weakening the knowledge base from which policy proceeds.

## Ideology and Research

These problems arising from the complications of competing interests and the complexities involved in predicting social events lead to the suggestion that we replace our former model of rational thought with a new one. In this modified model we would not expect research to lead from understanding to policy, but we would see each policy as a probe, which enables us to acquire new insights into the current nature of reality. Research would therefore contribute more modestly only to the adaptation of policies to changing or unpredicted events.

But this more modest model, also, proves faulty. The difficulty arises from our own processes of learning and organizing understanding. New information is assimilated into a paradigm that is remarkably persistent and resistant to change. Policy paradigms are a curious admixture of psychological assumptions, scientific concepts,

value commitments, social aspirations, personal beliefs, and administrative constraint. They do not constitute a social science theory in the sense of being able to organize disparate evidence and predict future patterns. They are more like personal belief systems, not entirely manifest, encompassing various contradictions rather than seeking to eliminate them. It seems obvious that we need to distinguish between personal and collective paradigms, but we do not understand much about the development of either.

The term paradigm is used here to suggest a working model of why things are as they are. It therefore often provides a guiding metaphor of how the world works which implies a general direction for intervention: it is more specific than an ideology or a system of beliefs but broader than a principle of intervention. The concept of the poverty cycle is an example of a policy paradigm. The imagery of a vicious cycle provides a working model of the causal interactions that are believed to result in poverty, a moral interpretation of the responsibility for intervening in this causal chain, and a guide to the interventions that are therefore relevant and right. It is a problem-solving framework, which implies values and benefits but also procedures, habits of thought, and a view of how society functions.

Typically we incorporate new sources of knowledge and information so that they are compatible with the policy paradigm we hold. And while it does appear that new paradigms will under certain circumstances achieve hegemony, the process by which this occurs is not well understood. Some will argue that it is not so much that people change their paradigms as that the people who hold unsatisfactory ones die out or are repudiated. Yet to suggest that individuals never change their paradigms is to overstate the case. Some individuals change their minds when there is strong invalidating information. Many

revise their sense of reality in response to depressions and wars. But whether the often ambiguous and incomplete information derived from social science research can also serve to modify the belief systems we hold would seem, in general, unlikely, unless we have only loose attachment to these beliefs.

Let us consider a specific example to illustrate how a paradigm can resist change. In cities throughout the United States there has been a substantial increase in educational expenditures for ghetto schools due to the introduction of compensatory programs. But studies of these efforts indicate that they have contributed little to changing the level of educational achievement as measured by standardized tests. A RAND study which reviewed educational reforms such as the augmentation of resources, improvements in the processes and methods of education, and variations in the organizational environment of the school, came to the conclusion that "Research has found nothing that consistently and unambiguously makes a difference in student outcomes.... Research has not discovered any educational practice that offers a high probability of success."[6] What can be said about these findings? After the methodological battles are aired, the implications remain stubbornly inconclusive, largely because the same evidence can be used to support sharply different views. Those who were initially committed to the reduction of educational inequality can argue that the findings demonstrate only that not enough has been done. Those who were initially skeptical that academic potential can be modified will say that the findings confirm their presumptions.

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6 Harvey A. Averch, et al., How Effective is Schooling? A Critical Review and Synthesis of Research Findings, Rand Corporation, December 1971, p. iv.

The interpretation of the impact of research on social policy is typically integrated with beliefs about the ability and responsibility of government to intervene in the process. Some hold the view that continuity of commitment must be sustained by government even when programs fail, because when we jettison programs we also abandon commitment to the ideals they represent. Moreover the programs may be rejected prematurely as failures: "Let us suppose that a man is drowning thirty feet from shore. A rescuer throws him ten feet of rope. He drowns. It would scarcely be logical to conclude: 'Rope is no use in the prevention of drowning.'"[7] Failure, according to this framework, usually demonstrates that the program had not tried hard enough, long enough, consistently enough, and with sufficient resources to accomplish its task. In his message to Congress in support of the Equal Educational Opportunities Act of 1972, President Nixon adopted something of this view:

> While there is a great deal yet to be learned about the design of successful compensatory programs, the experience so far does point in one crucial direction: to the importance of providing sufficiently concentrated funding to establish the educational equivalent of a "critical mass," or threshold level. Where funds have been spread too thinly, they have been wasted or dissipated with little to show for their expenditure. Where they have been concentrated, the results have been frequently encouraging and sometimes dramatic.

Others, however, begin with the belief that governmental intervention in most areas of social policy is typically ineffective, or worse still, that it generates more

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7 New York Times, February 27, 1973, "The Short Rope." This editorial rejects a statement in President Nixon's radio talk on "Human Resources" where the President asserts that money for social programs to aid the poor had flowed from Washington "in a seemingly inexhaustable flood." The Times argued that there was no flood and that "Every Great Society social program was seriously underfunded--at levels below Congressional authorization--because of spending on the Vietnam War between 1966 and 1969.... Some of the...programs" need more money, others need more time.

problems than it solves. They interpret information that reports the limited success of educational intervention, for example, only in such a way that it corroborates with what they already knew; and thus they recommend that the programs be ended or assigned a low fiscal priority. Government should do as little as it can politically get away with. If the data had shown that there was improvement, they might have raised the issue of the cost required to produce a benefit, or they might have explained that the success was due to "creaming." Positive findings are perhaps a bit more difficult to explain away than negative findings, but the difficulties are not insuperable -- just as negative findings can be discounted as the outcome of measurement errors, or the diffuseness of goals, or of an insufficient input. In the very frequent instance of inconclusive findings, different parties can choose which findings they will pay attention to. In these and other ways paradigms can be defended against new data. The conclusion to be reached from this example is that research findings may be used to support the view you already hold, but that if they do not support it, they need not be interpreted as invalidating it.

These conclusions are not limited to one field of social policy, nor are they fresh insights unnoticed by other observers. In his reflections on the vigorous academic debate over the busing of children to racially integrated schools and whether it affects their school performance, James Q. Wilson has formulated two general laws to cover all cases where social scientific evaluation judges public policy programs. He argues that either all interventions produce their intended effect or none do, depending on the prior beliefs held by those who conducted the inquiry. This anomaly arises largely because different standards of evidence and methods are applied, but also because the

some evidence is subjected to different interpretation and weighting.

> Studies that conform to the First Law will accept an agency's own data about what it is doing and with what effect; adopt a time frame (long or short) that maximizes the probability of observing the desired effect; and minimize the search for other variables that might account for the effect observed. Studies that conform to the Second Law will gather data independently of the agency; adopt a short time frame that either minimizes the chance for the desired effect to appear or, if it does appear, permits one to argue that the results are "temporary" and probably due to ... the reaction of the subjects to the fact that they are part of an experiment; and maximize the search for other variables that might explain the effects observed.[8]

## Social Policy Paradigms and Utilization of Research

Three obstacles that inhibit the influence of research on policy development have been briefly reviewed. The first two difficulties, namely, the difficulty of reconciling the ideals of democracy with the ideals of impartial rationality, and the complexity of the social events which research needs to understand and predict, have received attention in the literature. Neglected, but perhaps of greater importance, is the third problem that all knowledge must relate to a framework of interpretation, because information only makes sense in a framework that interprets it. If knowledge is to be useful in a collective way, politics aside, a widely shared viewpoint of interpretation is required. There are, however, only competing perspectives, and research cannot aid in choosing among them but makes sense only in terms of them. This conclusion offends our cherished allegiance to the view that learning from experience is not only necessary but possible. If policy commitments are only with difficulty informed by evidence, what can we count on? What can we do?

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8 James Q. Wilson, "On Pettigrew and Armor: An After Thought," The Public Interest, No. 30 (Winter 1973), p. 133.

The work of Kuhn, especially his concept that paradigms are replaced in response to crisis, may be relevant for understanding the contribution of social science research to established social policy paradigms. Kuhn argues that science does not develop only by the gradual accumulation of new knowledge, the correction of previous error, and the addition of new discoveries but by a series of crises in adaptation. Normal science proceeds, first, from puzzle solving within the context of an accepted paradigm that organizes disciplinary realities. Anomalies are discovered from the usual puzzle-solving activities,and these become increasingly hard to assimilate into the dominant paradigm. A scientific crisis emerges when scientists become aware that the paradigm's organizing capacity has collapsed and a substitute is at hand. Because there is a substitute available, abandonment of the earlier paradigm becomes possible. Otherwise, people will still cling to the earlier formulations. The crisis is followed by a scientific revolution in which a new integrative paradigm is constructed, usually by the powerful insight of a single man of genius -- although fundamental discoveries are seldom unique. What follows then is a struggle for acceptance, because the new paradigm offers a reformulation of the problem and a redefinition of the kinds of data that bear on the problem. In times of crisis, the scientific criteria of interpretation are themselves in doubt. The acceptance of the new paradigm is a strategic decision based on the judgment that it will contribute to a more complete understanding. As such, it is very much like a radical policy decision. Over time the new paradigm is itself subject to the same process of challenge and assault. Science, then, develops as an alternation of crises and sustained exploration of an established line of thought.[9]

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[9] Thomas Kuhn, The Spreading of Scientific Revolutions, 2nd ed., Chicago: University of Chicago Press, 1970,pp. 10-25. For criticisms of Kuhn, see I. Latakos and A. Musgrave, eds., Criticism and the Growth of Knowledge, Cambridge University Press, 1970.

Turning to social science, one can see a parallel with Kuhn's interpretation about the development of science. Knowledge about social issues, to be useful in a collective way, presupposes a widely accepted framework to interpret it. By analogy with Kuhn's analysis, we would expect:

1. That research is not generally designed to challenge the paradigm but to develop understanding within the framework of its assumptions. That is, if a policy does not work, the evaluation is not taken to discredit it but to refine and convert the particular application of the theory. The negative evaluation is itself challenged and fault found with the method by which the research conclusions were reached. This procedure should lead to continual improvements in the power and sophistication of the theory and more successful applications.

2. That this process will lead in time to a crisis of confidence in the theory when it begins to show more and more unaccountable inconsistencies with the evidence. A new theory, with different implications, a different sense of moral responsibility, a reconception of the problem, will then take its place and run the same course.

But does the relationship between research and social policy follow this pattern? Are there really social policy paradigms? Is there ever such a consensus of approach? And if these paradigms direct research, does research reciprocally undermine the paradigm? Science is a more or less self-enclosed system, where research is its whole life. But research is only one influence (perhaps a very small influence) on the determination of social policy. Two possible approaches to these questions are considered. The skeptical position leaves little scope for research to contribute to

policy; the cautiously optimistic view suggests a potential role for research and is preoccupied with ways that this role could be made more effective. Both perspectives warrant further analysis and debate. As with most competing paradigms, evidence can be marshalled in support of either interpretation. Choice between these orientations is a matter of faith, not truth. Pragmatically it may be advisable for anyone interested in policy research to assume, as an act of faith, that if the quality of his work is good, it will be worthwhile and useful in the long run, even if he cannot know whom it will influence or when. But there is no more sense in his worrying about the ultimate validity of his enterprise than in a research scientist worrying about the philosophical foundations of induction. It is much easier in practice to recognize good research -- its moral sensitivity, the coherence and power of its interpretation, the carefulness of its method, the quality of its evidence, and the imaginativeness of its insight -- than it is to explain why or for what purpose it is good. It seems useful to present both interpretations here and then to develop at further length the more optimistic one.

## A Skeptical View

Policy is determined by a complex perception of social, economic and political issues on which social science theory and empirical research findings are only one, rather obscure influence. According to the skeptical view, policy paradigms change in response to social, economic, and political changes -- that is, they change because they have to rationalize a different reality, not because research has revealed how wrong was the interpretation of earlier circumstances. Given changes in socio-political and economic realities, there is a scramble for new ideas, and a new paradigm

is sought which can serve as an organizing framework for the development of specific programs. This new policy paradigm is, of course, determined by ideological preferences as well as by social and political realities. But it is the changed reality that shapes acceptance of a different paradigm. Still, the older frameworks are not altogether forsaken: administrative pragmatism and eclecticism provide continuity with the past. Therefore, policy influences new theoretical formulations, inspires research, and fashions the relevance of the findings. However, the reverse relationship does not hold.

There is seldom, if ever, a consensus over policy: there are always competing paradigms. Thus, research on social policy never acquires the relative consistency of interpretations found in scientific work. For example, much of the writing on poverty has taken place within a conceptual framework that sees poverty as a function of inadequate opportunities or resources to establish a reasonable quality of life. As this paradigm is developed, it absorbs data about the lesser opportunities of the poor for advancement, their inadequate living conditions, their greater incidence of disease, and their lower life expectancy. All these observations extend and enrich the fundamental idea that poverty is injustice and that much of this injustice has to do with an inequitable distribution of opportunities and resources to alter life situations. But these same data, perhaps given different emphases, and organized with different observations, could contribute to the enrichment and development of still other paradigms. For example, poverty may be seen as another culture, a way of life in which, perhaps, having uncommitted time is more important than the amassing of human capital through education or job experience. Virtually the same data, given different weighting and organized differently, can serve both paradigms. This implies that the interpretation of research findings is

always controversial, because they are related to different paradigms held by different people, and there is no way of arbitrating between these interpretations by any rational method. It follows, then, that policy-related research never develops the same fundamental axes of interpretation, since it is not focused within a common framework of assumptions.

## An Optimistic View

The main planks in the skeptical platform are that evidence does not help in the social choice among competing frameworks of thought, and that research follows policy developments, and the framework of values which contains them, but the process is never reciprocal. The optimistic view holds that public policy typically evolves from what is initially a common framework of thought, without which there would be no action at all. Research in the short run contributes to policy when findings are consistent with the accepted framework. In the long run, research and theory contribute as well to alterations in the framework . As a policy-relevant social science develops in sophistication and experience, it will increasingly serve policy objectives. Studies of the utilization of commissioned and noncommissioned research in the decision-making process would, if an adequate time frame were adopted, reveal that there is more scope for high-quality social scientific investigation than the skeptics allege.

We may consider the optimistic position in more detail. It does seem clear that no consensual social theory is as well worked out as the dominant theories in the natural sciences. Nevertheless, there are paradigms and interpretive frameworks

that are dominant among political actions. They are translated into legislative programs that capture, at least for the moment, views held by large segments of the population. Policy does represent a consensual framework for action. T. H. Marshall argues that "Without a foundation of near consensus no general social welfare policy would be possible."[10] Such a consensus is an intrinsic part of most stable contemporary societies, although it is not possible to say with precision where the consensus comes from. Marshall believes that it is part of an autonomous ethical system that is neither the summation of individual preferences, as revealed in the market, nor the outcome of interest group policies mediated by the democratic process. By its nature, this system of values is intrinsically authoritarian and paternalistic.

While multiple and complex social policies exist among the subunits of government at all levels, the conflicts between them are nevertheless subordinated to the more general social policy that contains them. In periods when the consensus is strongest (e.g., after wars and economic reversals), policy flourishes. However, it languishes at those times when accord cannot be reached.

If policy is largely framed within a consensual framework, then the skeptics' position is overstated. Social science knowledge does contribute to the improvement of social programs when it is based upon widely shared values and when the political competition among vested interest groups is at a minimum. When social science inquiry does not attempt to demonstrate that values need to be changed, we would expect research findings to be very useful. And this is especially true for a field of settled

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10 T. H. Marshall, "Value Problems of Welfare Capitalism," Journal of Social Policy, Vol. 1 (January 1972), p. 20.

policy where the crucial task is that of administration and implementation, but it is also true for a field where policy makers define the research agenda.

The usefulness of research for policy varies, then, with the type of research undertaken, its source of financing, and its administrative relationship to the policy maker. Seen in these terms, the urgent question is how to reorganize and refinance research so that it can better serve policy objectives. When research is undertaken at a high degree of particularity it is most likely to fit a framework shared by political actors to meet the requirements of administration. One approach at reorganization takes the view that politicians and administrators should just specify the knowledge they require and should then purchase it from universities and other sources, paying a reasonable fee for a product called "information." Such commissioned studies are likely to create little tension between research and action, since the researchers have a contractual obligation to provide a product specified in advance. It is when these circumstances are satisfied that research is likely to be most used.

Such a situation holds when the primary task of the study is technical -- to devise ways to alter the known poor fit between available resources and accepted aims. For example, how can a city's fire service be reorganized so that it is more efficient and effective in its own terms ? Research is useful when it can provide better approaches for getting things done within the existing terms of reference of the bureaucracy. Parenthetically, we might note that it is the administrators of such programs who become the important consumers of research findings and not necessarily the executive branch which might commission it -- as became evident in the course of research conducted by the Rand Corporation for the City of New York.

Research can also be useful to policy when it sets out to discover negative side effects that can threaten the political acceptability of a program. Sometimes government will create ex ante studies which monitor whether it is achieving its purposes. For example, in 1971 the British Parliament decided to discontinue the provision of free school milk to children over seven years of age. To quell political opposition, the bill called for a dietary and clinical survey of children in selected areas to monitor the impact of the policy on the physical well-being of the children exposed to it.[11]

This type of research contrasts with the more typical arrangements where research proceeds from the social scientist's discipline rather than from immediate policy considerations. In such cases, the research questions, methods, and time requirements are not framed so as to take account of the political constraints of power and the bureaucratic constraints of mandate, and this accounts for their limited usefulness in the short run.

The Rothchild report on research policies for Britain seems basically to accept the assumption that these are the processes which limit the contribution of research policy. This report distinguishes between "basic research" and "applied research and development." Aside from a small fixed proportion of research funds to be allocated for freewheeling or basic research, most research funds available to scholars outside the government should be allocated by governmental departments acting as "customers" to academics acting as "research contractors." The customer will both specify the

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[11] The example is drawn from a stimulating memorandum prepared by Antony King, University of Essex, England, titled "On Studying the Impacts of Public Policies," January 1972 (mimeo), p. 2.

problem to be studied and review the methods of inquiry. The more immediate, as well as the long-range, requirements of government departments will shape the research agenda.[12] This policy accepts as a self-evident truth the proposition that research studies requested by administrators and politicians, and paid for by the government department that commissions them, will be useful to policy makers because they are based upon an intellectual perspective shared by both researchers and government. It follows from this viewpoint that if the research interests derive from a perspective which is not shared by government, the immediate usefulness of the findings will be in question. Only if the findings are so compelling or so graphic as to convince those who do not share the same framework of thought will this not be true. And for the reasons reviewed earlier, this seldom occurs. The less the framework is shared, the less immediate will be the impact of research on policy in the short run. However, this conclusion is based on an assumption about the stability of the dominant paradigm. When paradigms are in transition, research that challenges earlier frameworks may become more, rather than less, influential. (This will be reviewed later.)

But the approach expressed in the Rothchild report engenders consternation among those responsible for inaugurating research. It robs them of the autonomy they deem essential for independent and creative analysis; it substitutes short-term experimental gains for lasting and more fundamental insights, and it thus threatens what is most essential to science -- open, independent inquiry informed by the researchers' curiosity,

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12 "The Organization and Management of Government R & D," A Framework for Government Research and Development, London: H. M. S. O. (Cmnd. 4814), 1971.

with minimum external constraints. Moreover, the policy itself is untested, for it is not based upon any prior analysis of the relationship between research use and the nature of the contractual relationship with researchers.

An alternative approach for increasing the usefulness of research to government proceeds on a different assumption. It calls attention to the weakness of the process by which knowledge is diffused and relevant information assimilated. The knowledge needed by the policy maker may be available, but he does not have access to it either because he fails to recognize that it exists, or because the information is so technical that he does not appreciate its practical usefulness. What are needed are intermediaries who can effectively relate the four autonomous segments of a highly interrelated system -- that of politics where policy is made, technology where hard and soft innovations are born, professional and administrative practice where established policies are implemented, and research. David Donnison has expounded this view and recommends that "We must develop social networks of creative, well-trained scholars with a policy oriented turn of mind, capable of moving in and between the worlds of practice, politics and technology. They must be firmly rooted in the academic world -- whether they work in universities or not -- so that they keep in touch, intellectually speaking, with students and colleagues there."[13]

There are numerous variations on the general theme that policy makers and researchers must find ways to interact with each other. Some researchers view the policy maker as part of the research process and insist that both meet periodically

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13 David Donnison, "Research for Policy," Minerva, Vol. X, No. 4 (October 1972).

to discuss their tentative findings, so that users of the research product are involved in the process of study. The assumption is made that participation enhances the commitment to make use of the study results and to view research as a process and not simply as a conclusion.

On the other hand research may be viewed not as a cooperative venture in which it serves as an ally to established policies (whether by formal contractual relationship or by an informal process of communication and learning), but as part of a political process which involves disagreement and contention. The controversy may be accepted as a natural function of a well-working machine, as a factor contributing to the breakdown of the machine, so that a different type of operating mechanism may become necessary.

Research may serve as moral witness or as social criticism. When it does so, it accepts, in common with those studies commissioned by government, a shared set of goals and a shared conception of the instruments and processes for achieving those goals. In good Weberian tradition, the researcher maintains that the social scientist has no authority to challenge statements of purpose. His research is not trying to persuade those who hold different values or are committed to alternative means of achieving them. He is practicing only internal criticism of the means-ends chain because his aim is to demonstrate that those involved in a given policy have failed to understand the consequences of their own actions. This kind of research also assumes that once negative facts are known, everyone must accept their moral implications. It proceeds from the premise that there is a consensus of opinion about purposes, so that if the facts are valid, and the implications are judged undesirable, government

will act to rectify the conditions or account for its failure to do so. Research is thus used as a strategy of shame, to embarrass government to do what it accepts as morally right. Of course, the assumption may be faulty when government's declaration of intent turns out to be only symbolic; then exposure will not lead to effective action but to a scramble for new symbolic programs.

This strategy is not always applicable. Research can monitor whether policy is achieving what it has set out to do only if government has a policy to implement, that is, if it has established a definite, unambiguous course of action directed to a specific and definable aim. But this is seldom the case. Government typically lacks coherent policies. For example, it may spend money on in-patient health services, community care, and hospital construction; and these expenditures will lead to a very definite pattern of resource allocation within the field of medical care. But this pattern is brought about by the independent decisions of a wide variety of bodies, each made disjointedly, incrementally, and unrelated to each other. Government as a whole has no over-all resource allocation policy; hence research cannot demonstrate a mismatch between aims, underlying processes, and outcomes. In these circumstances, research may act as social critic calling attention to the failure of government to "consciously choose a particular course of action or a particular allocation of resources."[14] It cannot act as moral witness demonstrating how government has failed to honor its commitments, because there were no commitments; government had surrendered to a non-policy. Research as

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[14] Antony King, "On Studying the Impacts of Public Policies," op. cit.

social critic may, in addition, set out to show that the picture of reality implied by existing intervention is false, muddled, or inconsistent. The outcomes cannot be as intended, either because there was no initial intent or because the world works differently than imagined by the policy makers.

Research as moral witness and as social criticism is much harder for governments to assimilate, to accept, and to act upon in the short run than the consensual studies, in part because it challenges the conventional interpretation of reality and because it does not accept the constraints of resources and purposes under which governments act. But even here research does not question the values themselves, except indirectly by implying that something has gone wrong in the weighting and ordering of multiple and conflicting aims. But the governments do, over time, come to incorporate the views of their critics. Thus, research that is either consensual (contract research and intermediaries who make the supply and demand system for information work effectively) or contentious (moral witness and social critic) can influence the direction of policy.

The optimistic argument reviewed so far establishes only a limited terrain within the framework of a dominant paradigm in which research can contribute to shaping policy. But can more detached analysis that challenges the very assumptions on which policy operates also exert influence?[15]

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15 When research attempts to examine the values that underpin policy, one form it may take is that of a philosophical reappraisal of ethical and ideological perspectives rather than an empirical inquiry into facts and realities. Polemical essays such as Michael Young's, The Rise of the Meritocracy can be as insightful and revealing as more empirically based studies, because they spell out the logical consequences of pursuing a particular perspective.

Dominant policy paradigms are by no means static and unresilient. They may be resistent to invalidation, but they are also open to development. They evolve over time. And social science ideas, together with social, political and economic changes, play a role in the development of these policy paradigms. The process is a complicated one and it is difficult, if not impossible, to isolate the unique role of research and to disentangle whether research is a cause of policy or a consequence of it. The influence of research is often diffuse, oblique, and always embedded in the changes it reflects. The interplay of knowledge and ideals, political maneuver and intelligent problem-solving is bound to be very subtle, ambiguous, and complicated -- a subject which is itself an important theme for empirical research.

Schematically, it may be possible to represent one possible context in which research contributes to basic policy changes. First, there is a broad consensual framework, which represents what Parsons calls common values. These common values have an integrative effect upon society. Social processes are, by definition, a mode of integration of social action; the very fact that we have a society means that basic value differences among men will be worked out. Society is best viewed as potentially, mutually, beneficially interacting units. This consensus, in turn, helps to create a political context that shapes research priorities and helps to define the agenda of what is regarded as useful inquiry and acceptable social science theory. Values, politics, and research form a closely interwoven fabric. Over time there are discernible changes in the character of the fabric. These are brought about by technological changes, which, in turn, influence events as well as following them. Thus research is not simply a

passive servant to its economic and political masters. It molds, as well as is molded by, the consensual framework of values that prevail in society at any moment in time. In this process, social science research and theory help to evolve an ideology of thought; and this in turn influences the values that subtly alter the economic, social, and political context as well as being influenced by them. Social science may take the form of social criticism during one era, and its ideas may be repudiated for the moment because they threaten the status quo. But in other periods, especially during times of economic change and political unrest, there is a search for new ideas to help legitimate, make understandable, and direct these broader societal forces.

It seems that the optimistic view must assume an integrative concept of society defined in terms of common values, and it must reject the competing view that the mode of being of any society is predominantly a conflicting one. At any moment in time, it may be possible to show that a consensus of opinion has been achieved and this might lend support to the position that society is best viewed in an integrative mode. In the conflicting model of society it is assumed that a common value framework and stability are achieved through the imposition of one group's standards on another: integration can thus be interpreted as the dominance of one group over another. Power in society allows only the realization of the goals of the dominant group , and it frustrates the goals of the subservient one.

Two examples drawing on different time perspectives may illustrate how research can help to change policy paradigms. The first example, taken from the field of mental health, explores how the subtleties of the processes are worked out in the short run, over a period of a decade or so. The second, tracing the development of policy in regard to those who are economically destitute, looks at the same process over

a much longer period spanning two centuries. Robert Rapaport has suggested that the diffusion of social science knowledge may follow a biological analogy: new ideas, like new organs, may first be rejected by the body, but then may gradually experience semi-acceptance, until finally the new organ is incorporated. A theory to account for this process of repudiation and final embrace of social science knowledge has not yet been developed. Some who have observed the phenomenon feel that intermediaries and an informed network of colleagues play important roles. To understand the contribution of research and organizational policy, it is necessary to take into account the time lag between initiation and acceptance and the subtle process of rejection and partial or eventual acceptance, since this may take place over a period of several generations. [16]

A study undertaken at the Chestnut Lodge Sanitorium and eventually published by Stanton and Schwartz in their classic book, The Mental Hospital, illustrates how some of these processes in the diffusion of research findings can operate in practice. The study, by a sociologist and a psychiatrist, attempted to show the potential scope of "milieu therapy" for patients in a mental hospital. The analysis provided a critique of psycho-analytically oriented psychotherapy that emphasizes one-to-one relationships between a patient and his physician in isolation from the administrative and organizational milieu in which the therapy takes place. The researchers tried, in their inquiry, to document the ways in which the context undermines the therapeutic process. They isolated specific social processes, such as covert disagreement among the staff, and

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16 Robert N. Rapaport, "The Therapeutic Community Revisited: Some Aspects of Social Science Diffusion," mimeo, n.d.

were able to illustrate the effects on patient behavior. This research thus revealed an important flaw. It weakened the belief (the paradigm) that recovery from mental illness can be achieved solely through interaction between the patient and his therapist. This critique of the hospital's operating assumptions was so fundamental that the hospital as a whole could not accept its implications for the running of the institution, nor its implicit and explicit recommendations, without witnessing a major upheaval. Oddly, the research study, which received wide acclaim, was accepted as an important analysis by members of the staff of the host hospital, but its findings were ignored. The study was widely read by professionals in other institutions and by the next generation of sociologists and clinicians. There is little doubt that at least some people had already come to believe in the desirability of reaching a different balance between individual and milieu therapy. The introduction of new drugs around 1956 provided a new technology, and this could now make a different approach feasible. But in order to present a cogent argument for their views, they had needed empirical research that provided persuasive documentation. The Stanton and Schwartz book, published in 1954,[17] and other subsequent studies provided a useful ally. Together with subsequent research that tried to link social form and patient behavior, it helped influence the general climate of opinion by making available for the first time empirical evidence that could offer intellectual justification for a new approach. Although this point of view had always been in the intellectual climate, no evidence had been marshaled to demonstrate or articulate it. It was only after the study became known that the framework was

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17 Alfred H. Stanton and Morris S. Schwartz, The Mental Hospital, New York: Basic Books, 1954.

accepted as a serious therapeutic approach. Findings of the study came, in time, to play an important role in the education of clinically oriented personnel. Thus teachers served as intermediaries linking research to its potential users. Young people who went to work in state hospitals were influenced by the general notions underpinning the research, and they further developed its implications for the structuring of wards. Some of the younger psychiatrists who worked at the Lodge at the time the study was conducted, and were influenced by it, also carried the concepts to other hospitals and saw them implemented. They too played the role of intermediaries, spreading the approach to new settings. There is unfortunately no firm evidence that the hospital which initially rejected the findings came later to be influenced by them. But institutions are not monolithic. They do not react as a whole to innovative ideas. Apparently some members or units of the hospital came, in time, to accept certain aspects of the study; and several other milieu projects did follow the Stanton and Schwartz study. But it is difficult to interpret whether these reflected the interests only of specific staff members or acceptance of the findings and the general approach by the hospital as a whole.

As a second example of how studies contribute to changing policies, we can review the treatment of the poor. Early nineteenth-century political economy developed the view that those who accepted poor relief should be "less eligible" or more uncomfortable than the lowest paid independent laborer. This guiding metaphor was translated into legislation and embodied in the Poor Law Amendment Act of 1834. To implement this principle, a major policy innovation was required in Britain. A central authority and a small paid inspectorate set out to abolish the local practice of aiding the destitute in

their own homes (outdoor relief). Relief was to be offered only to those who agreed to enter a workhouse. These were then to be classified as able bodied and non-able bodied, and the able bodied were to be subjected to the principle of "less eligibility." The aim of the legislation was to drive the able-bodied poor back into the labor market where they could win personal dignity through work. The New Poor Law was interpreted as a final solution to the problem of pauperism, for it would reshape the moral character of the able-bodied poor.

These administrative reforms, and the critical ideas on which they depended, owed their origin to the social theories developed by Bentham and his followers. The Poor Law Reform of 1834 offers an example of the influence of a normative theory of political economy upon legislation at a time when the development of the factory system required a more mobile labor force to serve it. Theory helped to explain and to guide the public response to an industrial order organized around market principles of supply and demand.

But the New Poor Law of 1834 did not result in the promised efficiency. Research throughout the nineteenth century in England attempted to discover the anomalies in the New Poor Law paradigm, and it served as the nucleus upon which modern social policy grew. "The distant future lay with those whose empirical observations enabled them to repudiate the paralyzing fatalities of the principles of 1834 and the theory on which they rested."[18]

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18 O. R. McGregor, "Social Research and Social Policy in the Nineteenth Century," British Journal of Sociology, Vol. 8, No. 1 (March 1957), p. 148.

Policy-oriented social science in the mid-nineteenth century was narrowly empirical in focus, aimed at describing social conditions and measuring "the economic costs of the social wastage inherent in unregulated industrialism."[19] Its purpose was to discredit and to expose the inadequacies of the New Poor Law doctrine which established a rigid distinction between poverty and pauperism. Official statistics showed a steady decline in pauperism. The number of paupers on relief fell from 8.8 percent of the population in 1834 to 2.5 percent by 1900. These statistics suggested that improved poor law administration and a rigid standard of living had together combined gracefully to eliminate poverty as an urgent social problem.[20] Yet, the studies of Booth in East London (1887) and Rowntree in York (1899), based on what in their day were massive statistical surveys showed that about 30 percent of the population had incomes inadequate to maintain themselves. These facts lent support to a new theory of collective responsibility, which was further developed in the writings of the Webbs. The Webbs believed that citizenship should provide each individual with the right to a nationally imposed minimum standard for education and training, employment and income retirement, medical care, etc. Once the principle of universal entitlement was accepted, there were still competing options left open as to how these goals could best be satisfied. But the new guiding policy metaphor influenced the course of legislation in the early twentieth century and helped produce the Beveridge Report and the reforms of 1948, which ushered in the era of the welfare state.

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19 Quoted in O. R. McGregor, op.cit., p. 146.

20 Michael E. Rose, The Relief of Poverty 1834-1914, London: MacMillian Press, 1972, p. 15.

Once these reforms had been implemented, they too became the subject for new empirical investigation and criticism: Beveridge's approach failed to produce the guaranteed minimum it promised. New theory came to replace earlier conceptions of the minimum. Poverty is now viewed by at least some social scientists, such as Titmuss in Britain and Rainwater in the United States, as inequalities in the command of resources, and especially those inequalities that derive from the exclusion of specific groups from the dynamic and evolving life style, rights, and opportunities available to the average members of society. If the earlier principles were forged from studies that sought to document the wastage of unregulated industrialism, the new principles are evolving around issues of equity in the distribution of public resources: who pays and who benefits and who is excluded from established policies? Does the distribution of public largess multiply privileges or redistribute advantages? Empirical studies have provided preliminary documentation for the generalization that in the context of a democratic liberal political system it is the middle class that benefits most from the high-cost sectors of governmental intervention; and the broader the definition of social policy, the more nearly does this apply. The social policies that emerge from this prospective reject minimum subsistence as a relevant goal of public intervention and reject the conception of poverty as a failure to achieve subsistence standards, defined in absolute terms. When poverty is viewed as exclusion from membership in society, public policies become necessary which will attempt to alter the distribution of resources. The impact that these new ideas may have on future policy remains uncertain, but a debate along these lines has been launched.

This brief review of the development of policy paradigms in regard to poverty is broadly consistent with Kuhn's ideas about the way in which paradigms can be modified. The examples suggest that research evidence does help to make an assault on older, widely held policy paradigms and that these new findings make it harder to interpret knowledge in the accepted way and also contribute to the development of new paradigms. In this view, social science theory and empirical research play a definite part in the complex process by which society constructs its perceptions of reality, defines what its problems are, and determines what principles of intervention should guide its action and inaction.

## Are the Skeptics or the Optimists Right?

It is a mistake to assume that the matter is settled. Which is more valid, the skeptical or the optimistic view, depends on an act of faith rather than on evidence. More systematic research on research may suggest that the contribution of research to policy is limited, either because analysts have failed to provide useful answers to the questions about which government is concerned (most studies dispel myths rather than propose solutions), or because government cannot assimilate or use the knowledge, when it is available, for a variety of administrative and political reasons. However, those who have faith that research and social science can be used by government will continue to seek ways in which our understanding of social processes can be improved and the obstacles to utilization overcome.

But it is not only the policy maker's perspective which needs to be taken into account. The researcher too must come to terms with those who have power to make

policy. He can follow one of three broad strategies. He can proceed to work out his analysis in the abstract, unrelated to the political process, and without asking where the policy backing for his conclusions will come from. He can assume that there is and ought to be a division of responsibility between those who set out to describe reality and those responsible for action. But if this approach creates a situation where there are no takers, because the research findings are ignored by those who have power to make decisions, the researcher may then proceed in a different way. He may, for example, conduct his work in such a way as directly to influence those who have power, or, alternatively, interest groups in opposition to established power. But he may then have to give up some autonomy to buy influence. The only way to avoid this is for the researcher himself to enjoy a position of power (usually because he has achieved scientific prestige from other contributions), and thus be able to meet established power on an equal ground. If he enjoys a position of status, then what he says may not altogether be ignored. The researcher has only three choices with respect to power: he may ignore it, leaving it outside his considerations; he may subordinate some of his autonomy to power; or he may have an independent base of power won by his own prestige.

## A Modern Research Strategy in the Optimistic Tradition

We have reviewed three research strategies in which empirical findings contribute to the development of policy: a consensual approach, whereby government contracts for the information it wants or intermediaries make the knowledge market work better by bringing together knowledge producers and knowledge users; a contentious

approach, whereby research acts as moral witness for the failure of society to honor its commitments or as a social critic when government has established no commitments to be honored; and a paradigm-challenging approach, whereby the researcher acts independently of the established paradigm and tries to expose its fundamental weaknesses and to propose alternative principles of intervention, under the assumption that political support for the assault will follow at a later time. Each approach can contribute to policy. While research is typically undertaken within one or another of these frames of reference, it is however not uncommon for a single study to shift from one strategic level to another to accommodate the varied motives of the researcher. But the kind of evidence needed to advance each strategic position is different. If researchers want to be influential and to avoid personal frustrations they must be clear about the interpretive context in which their studies are framed, and also about the different levels of evaluation. This means that those who conduct empirical research in the hope of being useful in the development of policy must guard against two dangers. The first is the subtle transition from one style of analysis to another, so that the conclusions drawn will not follow from the original premise. For example, a researcher may attack a paradigm but nevertheless want to be of immediate use, which in most cases is not possible; thus he may be led to make a set of recommendations which are altogether irrelevant to his starting assumptions; or a researcher may refrain from using his radical perspective in fashioning his research design, but nevertheless impose it in his final appraisal of directions for action. The second danger that threatens the credibility of the analyst is the confusion of levels of analysis. For example, he may have evidence to show that a program does not work, from which he may inappropriately infer that the theory of intervention is misconceived or that the goals it espouses cannot be implemented.

These two difficulties often overlap because the style and level of analysis intercept. A consensual approach proceeds from agreed-upon aims; it asks whether policies, and the specific programs that implement them, work as intended. Program evaluation provides, of course, one of the basic ways that such an assessment can be made. A contentious approach proceeds in a more disputatious manner, searching for the very areas in society where our social ideals are not honored, not because we try and fail but because we do not make an effort in the first instance. A paradigm challenge occurs when the validity of a theory of intervention is brought under scrutiny.

How then do we move ahead to develop a viable social policy paradigm? Although little systematic work has been done on the study of complete paradigms which can guide us in the formulation of a specific social policy paradigm, it would appear that we need to address at least three main questions: (1) What are the main goals or purposes to be pursued? (2) What theories or principles of intervention might be pursued to achieve these purposes? (3) How can we determine whether programs are in fact consonant with these theories, and whether, in fact, they work as intended? An integration of the answers to these three questions will hopefully constitute the substance of a policy paradigm.

As we have suggested, the study of paradigms must consider not only the specific elements of the paradigms, and their relationship, but their dynamic interplay within the broader social, political, and economic contexts. The following section, however, is a more limited and circumscribed presentation. It examines the purposes, strategies, and programs of social policy, per se, without considering their relationship to the broader social setting in which they are located.

The Disparity Between Ideals and Realities

Since a social scientist has no claim to moral authority, what contribution can he hope to make to the question of social goals? One thing he can do is to undertake empirical studies to document the disparity between existing ideals and established realities. As a member of society he has an intuitive sense about the kinds of disparities which may be most acceptable politically. But whatever process he employs to select a problem for study, he proceeds on the assumption that policy should attempt to narrow the disparaties which are revealed. Of course, this works best for those ideals in society on which there is substantial accord. But is there agreement about our social ideals? While some measure of consensus about social aims does appear within a specific sector of social policy, it seldom obtains for all sectors. Decisions are typically made by autonomous departments without much attention to each other and without being subordinated to a single unambiguous policy objective. Governments seem uncommitted to a single embracing purpose but tend to seek multiple aims that are contradictory and conflicting. And yet despite this conflict of intentions and seeming absence of an explicit consensus and social purpose, those who hold an integrative view of society may after all be correct in believing that there is an elusive consensus on a deeper level. Superficially, we see a discordance of belief systems and ideals; the underlying agreement and integration in society is hidden and may take some effort to be identified. Thus, the social ideals and belief systems that underpin them are not self-evident; they need to be discovered. This is the first task of the researcher as critic.

I expect that there is no rigorous way that the analyst can tap the crucial issues of the day. But an examination of the way in which issues are brought to public attention

for action suggests that the pattern may be cyclical. In Britain, for example, in the mid-1950's, researchers and politicians were apparently preoccupied with the problems of the aged. This slowly gave way to a concern for poverty among children, then to the problem of low wages, and most recently public attention has been riveted on the circumstances of the disabled. Perhaps one process by which the researcher may uncover issues and illuminate their importance can be explained in the following way. When the treatment of special groups seems inconsistent with an implicit ethical standard, a diffuse dissonance is established which announces that the state of affairs is morally wrong. The researcher understands this dissonance from his role as citizen and from his moral sense of what offends him, and is thereby guided in developing his research questions.

The researcher then attempts to document the size of the disparity between shared ideals[21] and actual performance -- on the grounds that the case for change is more compelling if large numbers of persons are affected. Thus, quantitative studies of the incidence of hunger, poverty, ill health, chronic unemployment provide evidence about the seriousness of the gap between ideals and realities. These can then be used to argue that the disparity needs to be redressed. Pressures for reform are substantially strengthened if they assimilate not only the idea that something is morally wrong but also the idea that this state of affairs is socially dangerous. Pragmatism thus reinforces morality, each appealing to its own constitutency. While need and demand studies

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21 Some research tries to develop new social ideals; for example, some studies have tried to demonstrate that technological developments have made work redundant, so that we should prepare for a society that allocates work rather than leisure as a scarce resource.

may support a variety of paradigms, they tend mainly to reinforce those paradigms that arise from impatience with the status quo. This is because "need" oriented research always uncovers unmet needs. By definition needs are rarely fully satisfied and social ideals are almost never in accord with social reality. No one actually expects the real and ideal ever to be identical. We may note, however, that the evaluation of real programs presents the obverse problem. To start with ideals suggests that we may need to do more. To start with programs suggests that whatever we do fails, because research reveals the failure to achieve goals. Critics will note that the size of the problem is a function of the standard of need employed. In judging the scope of the problem, the crucial issue is the definition of standards.

Democratic political systems operate in such a way that they are responsive to strong political constituencies. Many public policies win support when they are initiated by interest groups that are articulate and powerful. Sometimes the interest group involved is mainly interested in keeping quiet the gains it has won, as in the case of business interests that have won generous oil depletion allowances. It sometimes seems as though it is a peculiarity of welfare policy that the case for its reform is expected to be substantive as well as political. Interest groups are therefore interested in marshaling whatever empirical evidence or persuasive argument they can develop to support their case. However, the kinds of arguments which the spokesmen for welfare programs develop to identify the state of dissonance will tend to influence mainly those with whom they share the same culture and values -- in short,the same paradigms. To widen political support,spokesmen for welfare programs tend to broaden their argument so that it will appeal to different groups. However morally compelling an argument may

be on grounds of equity and decency -- that the poor should have decent housing, because it is dehumanizing in an affluent society to subject individuals to a lower standard of well-being than that enjoyed by most members of society -- it is seldom politically persuasive. To win support, the case for decent housing is therefore stretched: good housing will reduce juvenile delinquency and enhance the capacity of children to make use of their schooling, because, among other things, it will provide them with a place to study. Those interested in reducing crime and expanding education are advised to support the case for better housing. But the validity of the argument becomes weaker as the search for political support is broadened. And in the end, the persuasiveness of reformers lies more in their ability to apply pressure than in the substance and moral justification of their arguments.

Another way to broaden support is to present appealing or dramatic imagery to those who hold different paradigms but who are not strongly committed to them, or those who hold conflicting paradigms simultaneously. New imagery may even affect those who are already committed to an opposing paradigm by shaking their conviction in the essential rightness of what they believe. Qualitative studies play an important role in this process. Scientific researchers rarely make effective use of such studies because they are limited by their own turgid prose and are seldom able to evoke a sense of affront and outrage. This sense of injustice is more effectively achieved by journalists and fiction writers. Their reporting can develop new imagery about reality and may influence people to examine or modify their paradigms. These vivid accounts can also evoke empathy for the poor, the sick, and the disenfranchised, and call attention to the negative social consequences of poverty, illness, and alienation.

Theories of Intervention

Having documented the failure to achieve ideals, those who wish to influence public policy must then consider what kind of theory of intervention can help reduce the disparity between ideals and reality. A policy paradigm is incomplete if it fails to specify how goals might be achieved. Research can confront existing theories of policy intervention, and it can also contribute to the development of new theories. But in either event, it will not be definitive in invalidating or in producing theory. Social theories reflect world views of how society functions, but policy paradigms are broader than scientific theories, which account for causal chains leading to particular events, because they contain normative as well as descriptive dimensions. They are also different from values about what constitutes a social good, because they embody not only an ideology of ends but an ideology of means which suggests how these ends can be realized. Oddly, they often contain only an implicit political theory as to how these means can be implemented. Whatever theory is developed, it must be at once ideological and empirically linked to real social processes. If it is to be politically possible, that is to say practical, it must be phrased in such a way that it is socially acceptable within whatever seems to be the arena of decision making.

Consider the paradigm which holds that an individualistic society should provide its members with the opportunity to step up the social ladder. The paradigm embodies the ideal of equalizing opportunities, and that of assuring that a permanent underclass does not develop whereby disadvantages are perpetuated from one generation to another. It should be noted that most studies of social, occupational, and income mobility have not been organized within a policy or action framework. Thus, they merely describe and analyze

the pattern of social stratification and social mobility within society, and are therefore of little immediate use in the formulation of specific policies. Policy does not necessarily flow logically from a review of patterns and trends. However, such descriptions do contain an implicit normative framework. Those interested in policy will search these studies for clues as to the factors that frustrate opportunity, so that action can be based on an interpretation of these processes.

The initial values of the paradigm, however, do not determine the final form of intervention. Are institutional barriers the crucial factor in inhibiting the equalization of opportunity, or is individual incapacity a more urgent obstacle? "Equalization of opportunity," after all, may function to eliminate the "incapable." Yet theories of intervention to encourage individual social mobility tend to favor either an individualist or a structuralist perspective, if for no other reason than that limited resources force a reluctant choice. It seems that a theory of intervention must follow one of two directions for policy. Each view "presumes a different conceptual framework, steers attention to different variables, poses problems of a different order, and suggests different methods of approach to solve these problems."[22]

The dominant theory of intervention in regard to equalizing opportunity holds that increased educational achievement by individuals is crucial to the alteration of oppor-

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22 Roland R. Warren, "The Sociology of Knowledge and the Problem of the Inner Cities," Social Science Quarterly, December 1971, p. 473. Logically, Warren observes, we may not need to make a choice: "Isn't it perfectly possible to consider poverty as an integral property of the social structure, while at the same time acknowledging... [that] individual people may experience serious social handicaps because of their poverty, and may need individual help to overcome these deficiencies?"

tunity, although the precise process by which this occurs is subject to different interpretations. The failure to acquire cognitive skills is most often assumed to be the critical factor. But recent research has attempted to challenge these accepted theories of intervention. A study by Christopher Jencks convincingly shows that the relationship between educational achievement, IQ, and future income is very marginal.[23] Jenck's study, by calling attention to dispersions within a given level of educational achievement and neglecting the difference in the means between groups, brings into question the belief that the educational establishment contributes greatly to inequalities in eventual class position, not to mention the assumption that there is a compelling relationship between IQ and economic success in later years. To trample so unrelentingly on cherished ideas without proposing alternative theories of intervention must create ideological confusion. It is not surprising that Jencks and his colleagues have been accused of being both too radical and too conservative; yet what seems most certain in the absence of a politically viable alternative theory of intervention is that their findings will very likely be assimilated by all paradigms without causing dramatic change, unless the ideals of equalizing opportunity are totally repudiated or a different theory of intervention posited.

An example of a different ideal is the proposition that the promotion of individual mobility is less essential than the promotion of collective mobility. This approach does not attempt to eliminate stratification, but it does seek to change the degree of inequality by shifting the position of the groups at the bottom relative to those at the middle

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23 Christopher Jencks, et al., Inequality: A Reassessment of the Effect of Family and Schooling in America, New York: Basic Books, 1973.

or top. It would accomplish this by raising their prestige levels, working conditions, salary levels, fringe benefits, or some combination of these approaches. This view directs attention not to the failure of the poor but rather to the failure of the mechanisms for allocating resources and prestige among different occupational and social groupings.

Yet another theory has been developed by theorists which calls attention to the existence of a dual labor market. It is argued that the low- and high-wage sectors are discrete and discontinuous. Low-wage employment is characterized by dead-end jobs in which there is little opportunity for on-the-job training and economic advances. As long as these jobs exist, certain types of workers -- the less well educated, youth, women, ethnic minorities -- will be attracted to them. Policies directed to changing the characteristics of the individual, such as their skill, educational achievement, and motivation for work must in the end fail if no effort is made simultaneously to alter the structure of job slots in the labor market. After all, if native workers refuse to accept these dead-end, low-paying, jobs that are subject to high levels of unemployment, industry will recruit migrant labor for whom these new jobs will represent an improvement in their relative economic position. The crucial task, then, is to alter the structure of employment by redefining tasks within a job, and a job within an occupation (an institutional theory of intervention) rather than the structure of individual opportunity (an individual theory of intervention.)[24]

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24 See, for example, Peter B. Doeringer, "Low Pay, Labor Market Dualism, and Industrial Relations Systems," Discussion Paper No. 271, Harvard Institute of Economic Research, January 1973.

Program Evaluation

Specific programs translate the broadly defined theory of intervention into concrete actions, and it is these programs that are the object of evaluation. The case for evaluative studies is compelling and perhaps self-evident. What, after all, could be more rational than to evaluate the outcomes of public policy? There is no sense in enthusiastically supporting policies or reform if we never find out whether they work. Yet, although the case for evaluation may be self-evident, here, too, we observe that prior paradigms tend to invade the methods of analysis and the interpretations of the findings.

One strongly held argument maintains that if we are to produce better information about which government interventions work and which do not, it is necessary to design social programs deliberately as experiments. Reliable and valid information about social processes and social programs can only be gathered through the experimental approach, where random selection is the crucial strategy. Social scientists must "prepare themselves so as to be methodologically ready for a future 'experimenting society.'"[25]

An alternative framework proceeds from the assumption that experimental evaluations miss certain processes and outcomes crucial to an understanding of any program intervention. One implication of this approach is that the study design is less important than the existence of a prepared and sensitive observer to the process.[26] The emphasis

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[25] Donald T. Campbell, "Critical Problems in the Evaluation of Social Programs," a paper prepared for the annual meeting of the Division of Behavioral Sciences, National Academy of Sciences, May 19-20, 1970, p. 1.

[26] Robert S. Weiss and Martin Rein, "The Evaluation of Broad Aim Programs: Experimental Design, Its Difficulties, and an Alternative," Administrative Science Quarterly, March 1970.

on observation rather than on research design responds to three problems encountered in traditional, experimentally oriented studies:

1. There is a tendency to concentrate evaluations on immediate, discrete, quantitative measures of change, even if outcomes cannot be measured in that way and even if they are not altogether related to the purposes of the intervention.
2. Evaluative studies have a tendency to eschew the long-term, slow-maturing, qualitative changes that are hard to measure in favor of short-term changes, even when such a procedure imposes alien goals on a program.
3. There is a tendency to evaluate programs in isolation from the context in which they must operate. For example, compensatory educational programs may be evaluated within the context of a particular school but ignore the external forces that impinge on it. Yet a school may go through a yearly financial crisis because it must balance its budget by April although state reimbursement funds are not decided until June. Teachers must be notified six months in advance of dismissal -- so they may be dismissed in April only to be rehired in June. Such a procedure could have a demoralizing effect on their morale. The negative aspect of such policies and procedures may far outweigh any positive contribution that the compensatory program could have on the children's educational achievements.

In brief, then, traditional evaluations yield uncertain findings because they are too crude and narrow in the aims they purport to measure and too short-term in their scope. Moreover, the attention to outcome alone neglects the broader institutional context in which the program operates, and this can nullify whatever positive results may be achieved.

Differences of belief do not only suggest competing methods of evaluation; but also conflicting ideas about the purposes and findings to emphasize in the evaluation. Peter Marris, commenting on the experiments with community action and model cities in the United States, and community programs in Britain, argues that "The distinctive quality of these experiments in social planning is their attempt to explore the adaptability of the process of government to demands: first that they should bring the people they serve into their counsel, as party to the discussion, negotiation and choice of policies, and secondly that they should integrate the functions of government intelligently around problems as a whole."[27] If the experiments are seen to be concerned with the malleability of government to innovation rather than with the development of strategies for reducing poverty, then, clearly, they must be judged by different standards. Measures of the performance of government rather than short-run achievements of the individual are required. This interpretation exposes the unresolved question concerning the purposes of experiments with broad social aims.

But even when there is general agreement about aims, and objective evaluations are undertaken to determine whether they have been met, at the crucial moment decisions about the continuation of a program may be judged by altogether new criteria. Thus, programs which actually succeed in meeting their purposes may nevertheless become unacceptable. For example, it is widely assumed that the experiments with community-controlled schools were rejected because they failed, but in fact "The decision to abolish the experimental districts was made in the absence of data on the effects of this experiment

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27 Peter Marris, "Experimenting in Social Reform," mimeo., 1972, p. 6.

on schools and children."[28] Marcia Guttentag argues that community control, which has been widely acknowledged as a failure, was in fact, based on findings of later research, a success. One of the aims (and many have argued, the prime aim) was to change the behavior, aspirations, and attitudes of ghetto residents through a strategy of participation. People change as they try to change their world. The Harlem experiment with community-controlled schools created deep conflict between professionals and consumers, but it also changed the aspirations of the adults and increased the educational achievement of the children who participated in it.

## Conclusion

Each type of study yields different information and is responsive to a different question. There is a danger that we may shift our levels of analysis and draw conclusions from inappropriate sources of data. It is, after all, difficult to determine when the theory of intervention is right but the program implementation is faulty, or when the theory is wrong because the ideals which led to the theory are miscast. At least three different options are possible: (1) The ideals were right but the implementation was inappropriate, perhaps because the design of the program was faulty, or the level of commitment was insufficient, or the critical threshold was not reached (the medicine didn't work because the dose was too weak). (2) The ideals were right, but the theory of intervention was wrong. Education, for example, is not a key to the equalization of

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28 Marcia Guttentag, "Children in Harlem's Community Controlled Schools," Journal of Social Issues, Vol. 28, No. 4 (1972), p. 18.

opportunity and income because the link between education and income is very elusive and indeterminate in a society whose occupational structure is changing. (3) The ideals themselves are impractical: no theory of intervention could achieve them and new ideals are needed. Banfield has argued, for example, that the effort to alter the time preferences of ghetto adults, that is, their ability to plan for the future, is a self-defeating task and that new and more realistic goals are required.[29]

In evaluating a policy paradigm it is important to be able to assess whether the ideal, or the theory of intervention, or the program which translates that theory into a specific course of action is defective.

Program evaluations can only say something about program implementation. Many evaluative studies of the poverty programs were of limited use because they only inquired whether the programs worked or not -- these "go-no-go" studies could not suggest how to make the programs work better. When the ideals remain firm and the theory of intervention is unaltered, research seems useless if it is unable to suggest areas for specific program improvement. For example, so long as there has been a continuing commitment to the legitimate goal of helping youngsters to compete on equal terms in school, and it has been assumed that early education is important in shaping academic achievement and that ability is related to future income, negative findings about the Head Start program have simply produced a diffuse sense of dissatisfaction and frustration, discrediting both the value of research and the program.

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29 Edward C. Banfield, The Unheavenly City, Boston: Little-Brown, 1970.

Somehow evaluative studies seem, in the end, almost always to react against the reform itself, to support the preconceptions of those who believed to begin with that nothing works. But from these observations we cannot conclude that we should be satisfied with the reform without evaluation. Few would argue that reform is an act of faith and that what matters is the faith and not the outcome. If we are concerned about outcomes, then we must see that programs do what they are intended to do. But because evaluations seem so consistently to assault the reform itself, there is the danger that negative findings may give premature and unwise support to the cynical and the reactionary and may discourage and demoralize the idealistic.

As we suggested earlier, if one is interested in criticizing the theory of intervention, rather than the specific program, it is necessary to look at processes and try to examine whether events occurred as they were supposed to. That is to say, were the inputs to be evaluated those called for by the theory? For example, if community action theory holds that all interested parties should collectively define the problems and priorities of ghetto residents, and thereby create new programs out of this assessment, then the test of the theory requires a study of these processes. We would want to know whether things worked out according to program intention. Such a review might show that the program did not arise from the processes implied in the theory but was imposed ad hoc. And so the validity of the theory cannot be in question because the theory was never implemented, and hence its effectiveness cannot be judged. The normal support for outcome studies and rejection of process studies show yet again how the study of the effectiveness of policy is itself subjected to undetected paradigms.

If one wants to question ideals rather that the theory or the programs that are the objects of the evaluative study, then research needs to re-examine the assumptions on

which the ideals are based. For example, if structural unemployment persists, so that a substantial portion of the population is always unemployed, the ideal of equalizing opportunity can only mean equalizing the risk of being unemployed.

The strongest conclusion to be reached, then, is that we should not draw conclusions from the results of one kind of research when the results actually follow from another type of research. Social policy research is at great risk of being totally inconclusive because it moves too quickly between ideals, theories of intervention, and program specifics when no such progression can rightfully be drawn from the research. To use research productively, it is important to be clear at what level of analysis the research is relevant. There is a danger of drawing inappropriate conclusions about ideals and theories when the data relate only to the implementation of programs.

I have tried in this paper to illustrate the resistance of personal paradigms to change and the stubbornness of categories of thought despite alternatives suggested by research. Skeptics take this as evidence that only a weak link is possible between research and policy. Optimists argue that such a conclusion is premature. Research undertaken within a common framework of thought can both by contention and consensus influence the development of policy; moreover research and theory can help reshape policy paradigms as well, but their contribution cannot be isolated from the many other forces that impinge on decisions. I have set out the arguments in support of each position as impartially as I can. In the final section I have tried to illustrate how prior-held frameworks shape the ideals sought (the skeptic's view), the theories of action proposed, and the evaluations undertaken. The optimists, of course, reject this view and point to the many examples in the short and long run where research does appear to contribute to policy. But the process by which this occurs is a complex one. More importantly, I have tried to show that policy-

oriented research is in danger of falling into disrepute when researchers inappropriately shift among the three levels of analysis discussed. I am therefore a skeptical optimist. Self-conscious awareness of the limitation of each view permits one to avoid being the victim of either.